LETTERS TO THE EDITOR.

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The Principle of Similitude.

DR. RIABOUCHINSKY directs attention concisely in NATURE (July 29, p. 591) to an important point, which must have arrested the notice of readers of Lord Rayleigh's weighty exposition and illustration of the scope of the method of dimensions, as an instrument of precision in the analysis of physical problems. The example under consideration was the cooling of a hot wire by a stream of air passing across it. The point is that temperature, although in ultimate analysis it must be expressible in terms of the three fundamental dynamical entities—mass, space, and time—can yet be in that problem considered *effectively* as a fourth independent entity, thus vastly increasing the information derivable from comparison of dimensions. In the formal analysis of mere diffusion or conduction this is clearly valid, for the dynamical aspect of temperature is not involved.

In so far as thermodynamic considerations, such as work of expansion, etc., are of secondary importance in the analysis of the convection from a hot wire (as is the case so long as only an adhering surface layer of the gas need be taken as operative[1]), the same principle will apply approximately there. But in a problem the content of which is mainly thermodynamic, the relations will be far more complex. In fact there is nothing transcendental about dimensions; the ultimate principle is precisely expressible (in Newton's terminology) as one of *similitude*, exact or approximate, to be tested by the rule that mere change in the magnitudes of the ordered scheme of units of measurement that is employed must not affect sensibly the forms of the equations that are the adequate expression of the underlying relations of the problem.

J. L.

Cambridge, July 31.

THE question raised by Dr. Riabouchinsky (NATURE, July 29, p. 591) belongs rather to the logic than to the use of the principle of similitude, with which I was mainly concerned (NATURE, March 18, p. 66). It would be well worthy of further discussion. The conclusion that I gave follows on the basis of the usual Fourier equations for conduction of heat, in which heat and temperature are regarded as *sui generis*. It would indeed be a paradox if the further knowledge of the nature of heat afforded by molecular theory put us in a worse position than before in dealing with a particular problem. The solution would seem to be that the Fourier equations embody something as to the nature of heat and temperature which is ignored in the alternative argument of Dr. Riabouchinsky.

August 2. RAYLEIGH.

The Probable Error of the Amplitudes in a Fourier Series obtained from a Given Set of Observations.

To express a periodic variation by one or more terms of a Fourier series is such a common and convenient method that it is of importance to know the extent to which the constants are trustworthy. This is particularly the case when the quantity is subject to a large casual and therefore non-periodic variation as well.

Suppose there are n evenly spaced observations, $y_0, y_1, y_2, \dots y_{n-1}$, then confining attention to one term only, y may be expressed by the relation $y = a \sin (x - \alpha)$. This may be written

$$y = p \sin x + q \sin\left(x + \frac{\pi}{2}\right)$$

and the amplitude a is given by $a = \sqrt{p^2 + q^2}$.

The standard error of a therefore depends on that of p and q. To obtain p we write—

$$p = y_0 \sin 0 + y_1 \sin \frac{2\pi}{n} + y_2 \sin \frac{4\pi}{n} + \dots + y_{n-1} \sin \frac{n-1}{n} 2\pi$$

whence, if σ is the standard error of the y's, it is easy to prove that $\sigma\sqrt{2}/\sqrt{n}$ is the standard error of the p or q. This is also the standard error for any higher order term depending on the n observations.

From the way in which p and q are obtained it is obvious they may be positive or negative; the sign depends, in fact, upon the origin chosen, and their mean value obtained from many samples would be zero. But even in the case of there being no period at all, and the y's being purely casual, it is very improbable that both p and q should be zero. In this special case the amplitude a is really zero, but it is almost certain, since it equals $\sqrt{p^2 + q^2}$, not to be given as zero. Its mean square is plainly equal to twice the mean square of p or q, but this is not the square of the standard error, because it is not taken about the mean value.

To ascertain the general magnitude of the error the following plan has been adopted. From a sort of roulette board of 100 compartments, 500 pairs of numbers have been taken, the numbers on the board being arranged to have a standard deviation of 10, and a distribution as nearly normal as the limit of 100 numbers permits. From these pairs, representing the p and q of the sine curve, 500 amplitudes have been found. The result gives a mean amplitude of 12·5, a standard deviation of 6·5, and a distribution such that 4 per cent. exceed the value 25.

Similar results from 500 other pairs, but with a genuine sine curve of amplitude 10 superadded, give a mean amplitude of 16·5, a standard deviation of 8·0, and a distribution such that 8 per cent. are below 5, and 5 per cent. above 30.

From theoretical considerations it is apparent that as the periodic part of the variation becomes large compared with the casual part, the mean amplitude will still exceed, but will approximate to, the genuine amplitude, and the standard error will approximate towards that of p or q.

Suppose, then, that we have a set of n well-distributed observations, and that nothing is known about them in regard to their periodic variation. Let their standard deviation be σ. Now let the first three terms of the Fourier series be found in the usual way, and let the amplitudes be a_1, a_2, and a_3, the question that arises is, to what extent do a_1, a_2, and a_3 represent genuine periodic variations? The standard deviation σ may be due entirely to the periodic variations, or may be purely casual. In the former case it is easily proved that $\sigma = \sqrt{a_1^2 + a_2^2 + a_3^2}/\sqrt{2}$, and if this relationship is approximately satisfied, the larger amplitudes may be accepted as correct. But if the deviation σ of the observations is distinctly larger than that produced by the periodic part, the casual part of it will lead to error in the a_1, a_2, and a_3, and unless an amplitude when compared with $\sigma\sqrt{2}/\sqrt{n}$ is well outside the limits for the special values given above, it need not be significant.

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[1] *Cf.* also L. V. King, on hot-wire anemometry, *Phil. Trans*, A 214 (1914), p. 373.

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It follows that the amplitude of any term in a sine curve deduced from only a moderate number of observations must be received with caution until an investigation of its probable error has been made, and this remark refers especially to the second and higher terms where the amplitudes are so often small.

W. H. Dines.

Benson Observatory, Wallingford, July 29.

ON THE CHARACTER OF THE "S" SOUND.

SOME two years ago I asked for suggestions as to the formation of an artificial hiss, and I remarked that the best I had then been able to do was by blowing through a rubber tube nipped at about half an inch from the open end with a screw clamp, but that the sound so obtained was perhaps more like an *f* than an *s*. "There is reason to think that the ear, at any rate of elderly people, tires rapidly to a maintained hiss. The pitch is of the order of 10,000 per second."[1] The last remark was founded upon experiments already briefly described[2] under the head "Pitch of Sibilants."

> Doubtless this may vary over a considerable range. In my experiments the method was that of nodes and loops (*Phil. Mag.*, vol. vii., p. 149 (1879); Scientific Papers, I., p. 406), executed with a sensitive flame and sliding reflector. A hiss given by Mr. Enock, which to me seemed very high and not over audible, gave a wave-length (λ) equal to 25 mm., with good agreement on repetition. A hiss which I gave was graver and less definite, corresponding to $\lambda = 32$ mm. The frequency would be of the order of 10,000 per second, more than 5 octaves above middle C.

Among the replies, publicly or privately given, with which I was favoured, was one from Prof. E. B. Titchener, of Cornell University,[3] who wrote:—

> Lord Rayleigh's sound more like an *f* than an *s* is due, according to Köhler's observations, to a slightly too high pitch. A Galton whistle, set for a tone of 8400 v.d., will give a pure *s*.

It was partly in connection with this that I remarked later[4] that I doubted whether any pure tone gives the full impression of an *s*, having often experimented with bird-calls of about the right pitch. In my published papers I find references to wave-lengths 31·2 mm., 1·304 in. = 33·1 mm., 1·28 in. = 32·5 mm.[5] It is true that these are of a pitch too high for Köhler's optimum, which at ordinary temperatures corresponds to a wave-length of 40·6 mm., or 1·60 inches; but they agree pretty well with the pitch found for actual hisses in my observations with Enock.

Prof. Titchener has lately returned to the subject. In a communication to the American Philosophical Society[6] he writes:—

> It occurred to me that the question might be put to the test of experiment. The sound of a Galton's whistle set for 8400 v.d. might be imitated by the mouth, and a series of observations might be taken upon material composed partly of the natural (mouth) sounds and partly of the artificial (whistle) tones. If a listening observer were unable to distinguish between the two stimuli, and if the mouth sound were shown, phonetically, to be a true hiss, then it would be proved that the whistle also gives an *s*, and Lord Rayleigh would be answered.
>
> The experiment was more troublesome than I had anticipated; but I may say at once that it has been carried out, and with affirmative result.

A whistle of Edelmann's pattern (symmetrical, like a steam whistle) was used, actuated by a rubber bulb; and it appears clear that a practised operator was able to imitate the whistle so successfully that the observer could not say with any certainty which was which. More doubt may be felt as to whether the sound was really a fully developed hiss. Reliance seems to have been placed almost exclusively upon the position of the lips and tongue of the operator. I confess I should prefer the opinion of unsophisticated observers judging of the result simply by ear. The only evidence of this kind mentioned is in a footnote (p. 328): "Mr. Stephens' use of the word 'hiss' was spontaneous, not due to suggestion." I have noticed that sometimes a hiss passes momentarily into what may almost be described as a whistle, but I do not think this can be regarded as a normal *s*.

Since reading Prof. Titchener's paper I have made further experiments with results that I propose to describe. The pitch of the sounds was determined by the sensitive flame and sliding reflector method, which is abundantly sensitive for the purpose. The reflector is gradually drawn back from the burner, and the positions noted in which the flame is unaffected. This phase occurs when the burner occupies a *node* of the stationary waves. It is a place where there is no to and fro *motion*. The places of recovery are thus at distances from the reflector which are (odd or even) multiples of the half wave-length. The reflector was usually drawn back until there had been five recoveries, indicating that the distance from the burner was now $5 \times \frac{1}{2}\lambda$, and this distance was then measured.

The first observations were upon a whistle on Edelmann's pattern of my own construction. The flame and reflector gave $\lambda = 1·7$ in., about a semi-tone flat on Köhler's optimum. As regards the character of the sound, it seemed to me and others to bear some resemblance to an *s*, but still to be lacking in something essential. I should say that since my own hearing for *s*'s is now distinctly bad, I have always confirmed my opinion by that of other listeners whose hearing is good. That there should be some resemblance to an *s* at a pitch which is certainly the predominant pitch of an *s* is not surprising; and it is difficult to describe exactly in what the deficiency consisted. My own impression was that the sound was too nearly a pure tone, and that if it had been quite a pure tone the resemblance to an *s* would have been less. In subsequent observations the pitch was raised through

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[1] Nature, vol. xci., p. 319, 1913.

[2] *Phil. Mag.*, vol. xvi., p. 235, 1908. Scientific Papers, V., p. 486.

[3] Nature, vol. xci., p. 451, 1913.

[4] Nature, vol. xci., p. 558, 1913.

[5] Scientific Papers, I., p. 407; II., p. 100.

[6] Proceedings, vol. liii., August—December, 1914, p. 323.

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